friends by the help of a loadstone." Strada's remarkable lines are also quoted and translated in Mr. George Dodd's account of "Railways, Steamers, and Telegraphs: a Glance at their Recent Progress and Present State" (Chambers, 1867). Mr. Jevons found allusions to a magnetic telegraph running through many scientific, or quasi-scientific, works of the sixteenth and seventeenth centuries. Sir Thomas Browne, in his "Pseudodoxica Epidemica," says:—"The conceit is excellent, and, if the effect would follow, somewhat divine;" and he speaks of it as a conceit "whispered thorow the world with some attention, credulous and vulgar auditors readily believing it, and more judicious and distinctive heads not altogether rejecting it." Sir Thomas, it would seem, submitted the matter to experiment, but found that though the needles were separated but half a span, when one was moved "the other would stand like Hercules' pillars."

Joseph Glanvill, in his *Scepsis Scientifica* (1665), discusses the objections of Sir Thomas Browne, and concludes that "there are some hints in natural operation that give us probability that it is feasible." How can we read without wonder these words written by Glanvill more than 200 years ago? "Though this pretty contrivance possibly may not yet answer the expectation of inquisitive experiment, yet 'tis no despicable item that by some other such way of magnetick efficiency it may hereafter with success be attempted, when magical history shall be enlarged by riper inspections; and 'tis not unlikely but that present discoveries might be improved to the performance." It is evident that Glanvill treats the matter quite seriously as a scientific possibility. The Marquis of Worcester probably refers to the magnetic telegraph when he speaks of "intelligence at a distance communicative, and not limited to distance, nor by it the time prolonged." (Dirck's "Life of Worcester," p. 357.)

Mr. Jevons tried to trace these notions to the first inventor, but, as might be expected, without much success. Strada attributed the invention to the celebrated Cardinal Bembo, the secretary of Leo X., but as Bembo (who died in 1547) was a historian and literary character, it is hardly likely that he would originate a scientific conception of the sort. The earliest books in which Mr. Jevons found allusions to a magnetic telegraph is the *Natural Magic* of Baptista Porta, published in 1589. In the seventh book he describes the "wonders of the magnet," saying in the preface, "I do not fear that with a long absent friend, even though he be confined by prison walls, we can communicate what we wish by means of two compass needles circumscribed with an alphabet." In the eighteenth chapter of the same book, he describes the experiment of putting a magnet under a table and moving thereby a needle above the table. This experiment, as Porta remarks, was known to St. Augustine, and an exact description will be found in his "De Civitate Dei," a work believed to have been begun A.D. 413. It seems probable that this passage in St. Augustine suggested the notion either to Porta, Bembo, or some early Italian writer, and that thus it came to be, as Sir Thomas Browne says, "whispered thorow the world."

Mr. William E. A. Axon refers to the passage in Strada in which he supposes the loadstone to have such virtue that "if two needles be touched with it, and then balanced on separate pivots, and the one be turned in a particular direction, the other will sympathetically move parallel to it. He then directs each of these needles to be poised and mounted on a dial having the letters of the alphabet arranged round it. Accordingly, if one person has one of the dials and another the other, by a little pre-arrangement as to details, a correspondence can be maintained between them at any distance by simply pointing the needles to the letters of the required words." The date of the first edition of Hakewill's "Apologie or Declaration of the Power and Providence of God in the Government of the World" is 1627; but the work of Strada's from which he quotes was published ten years earlier. Famianus Strada was born at Rome in 1572, and his "Prolusiones Academicæ et Paradigmata Eloquentiæ" appeared at Rome in 1617. Several editions of his "Prolusiones" have been printed in this country. The particular poem referring to the loadstone has been translated into English, and is printed in "The Student; or, Oxford and Cambridge Miscellany," 1750. The passage is referred to by Addison in a paper in the *Spectator*, No. 241, and in the *Guardian*, No. 119. In the former of these he adds: "In the meanwhile, if ever this invention should be revived or put in practice, I would propose that upon the lover's dial-plate there should be written not only the four-and-twenty letters, but several entire words which have always a place in passionate epistles: as flames; darts; die; language; absence; Cupid; heart; eyes; hang; drown; and the like. This would very much abridge the lover's pains in this way of writing a letter, as it would enable him to express the most useful and significant words with a single touch of the needle."

The subject is an interesting one, and seems to us well worth being followed out.

EVOLUTION OF NERVES AND NERVO-SYSTEMS[1]

II.

As the Medusæ are thus the lowest animals in which a nervous system has yet been discovered, we have in them the animals upon which we may experiment with the best hope of being able to elucidate all questions concerning the origin and endowments of primitive nervous tissues. I have therefore spent much time and labour, both last year and this year, in cultivating this field of inquiry; and as it is a field whose ground had never before been broken, and whose fertility has proved itself prodigious, it is not surprising that I should have reaped a rich harvest of results. So far as these results have any bearing on the general theory of evolution, their character is uniformly such as that theory would lead us to expect. For if I had two hours at my disposal instead of one, I might mention a number of facts which tend to show, in a very striking manner, that the primitive nervo-muscular tissues of the Medusæ, in respect of their physiological properties, present unmistakable affinities, on the one hand with the excitable tissues of certain plants, and on the other hand with the nervo-muscular tissues of higher animals. But not having time to go into this matter, I shall on the present occasion restrict myself to describing such of my results as tend to substantiate Mr. Herbert Spencer's theory concerning the mode in which nerves and nervo-systems have been evolved. And I adopt this course, not only because I feel that any facts bearing on so important a subject cannot fail to be of interest to all intelligent persons, but also because I think that this is a place best suited for publishing the somewhat speculative inferences which I have drawn from my facts. If these inferences are correct, their philosophical as well as their scientific influence will be great and far-reaching; but until they shall have been more completely verified I have not thought it desirable to adduce them in my communications to the Royal Society. Referring, therefore, those among you who may be interested in the research as a whole to the *Philosophical Transactions*, I will now invite your attention to a connected interpretation of some of the facts that it has yielded—an interpretation which I here publish for the first time.

To begin, then, with this diagram, Fig. 2. It represents *Aurelia aurita*, with its polypite cut off at the base, and the under, or concave, surface of the bell exposed to view. The bell, when fully expanded as here represented, is about the size of a soup-plate, and in it all the ganglia of the margin are collected into the eight marginal bodies; so that on cutting out these eight marginal bodies total paralysis of the bell ensues. But although the bell is thus paralysed as to its *spontaneous* movements, it continues responsive to stimulation; for every time you prick or electrify any part of the contractile sheet, a wave of contraction starts from the point which you stimulate, and spreads from that point in all directions as from a centre. Such contractile waves, at ordinary temperatures, travel at about the rate of a foot and a half per second; and the important question with regard to them which we shall have to consider to-night is this—Are they merely of the nature of muscle-waves, such as we see in undifferentiated protoplasm, or do they require the presence of rudimentary nerve-fibres to convey them—the *stimulus* wave in the rudimentary *nerve*-fibres progressively causing, as it advances, the *contractile* wave in the rudimentary *muscle*-fibres?

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[1] Abstract of a Lecture delivered at the Royal Institution on Friday evening, May 25, 1877. By George J. Romanes, M.A., F.L.S., &c. Continued from p. 233.

Now the great argument in favour of these contractile waves being muscle-waves, and nothing more, is simply this—that the contractile tissue is able to endure immensely severe forms of section without the contractile waves in it becoming blocked. For instance, when the bell of *Aurelia* is cut as here represented, Fig. 3, and any part of the circle is stimulated, a contractile wave radiates from the point of stimulation just as it did before the cuts were introduced, notwithstanding the wave has now to zig-zag round and round the ends of the overlapping cuts. Similarly, if instead of employing artificial stimulation, a single ganglion (*g*) be left *in situ* while all the other seven are removed, contractile waves will radiate in rhythmical succession from the single remaining ganglion, and course all the way round the disc. Now this experiment seems to prove that the contractile waves depend for their passage, not on the conductile function of any primitive nervous net-work, but on the protoplasmic qualities of the primitive muscular tissue. The experiment seems to prove this, because so severe a form of section would seem of necessity to destroy the functional continuity of anything resembling such a nervous net-work as we observe in higher animals.

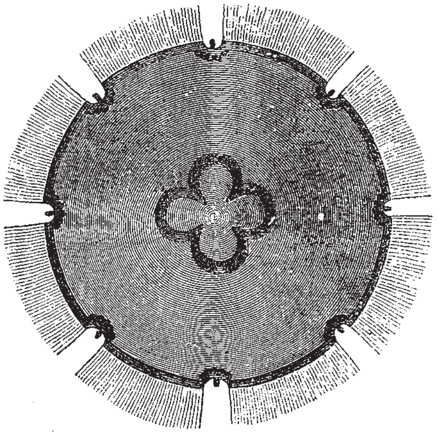

Fig. 2.

Here, again, Fig. 4, is another form of section. Seven marginal bodies having been removed as before, the eighth one was made the point of origin of a circumferential section, which was then carried round and round the disc in the form of a continuous spiral—the result, of course, being this long riband-shaped strip of tissue with the ganglion (*g*) at one end, and the remainder of the swimming-bell at the other. Well, as before, the contractile waves always originated at the ganglion; but now they had to course all the way along the strip until they arrived at its other extremity, and as each wave arrived at that extremity it delivered its influence into the remainder of the swimming-bell, which thereupon contracted. Hence, from this mode of section as from the last one, the deduction certainly appears to be that the passage of the contractile waves cannot be dependent on the presence of a nervous plexus; for nothing could well be imagined as more destructive of the continuity of such a plexus than this spiral mode of section must be.

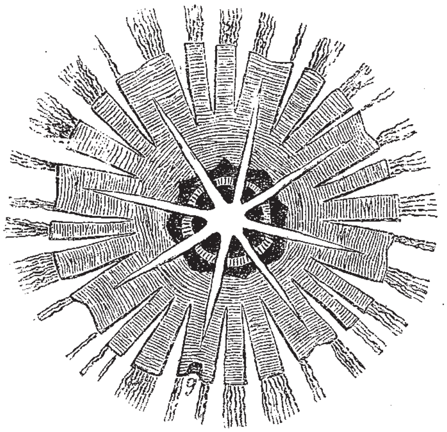

Fig. 3.

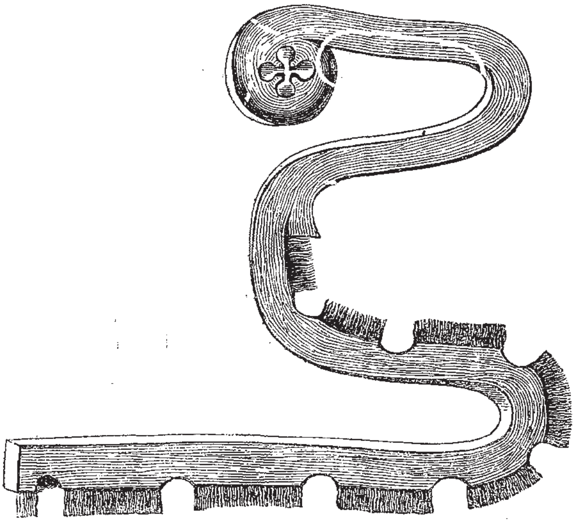

Fig. 4

Nevertheless there is an important body of evidence to be adduced on the other side; but as I can only wait to state a few of the chief points, I shall confine my observations to the spiral mode of section. First of all, then, I have invariably found it to be the case that if this mode of section be carried on sufficiently far, a point is sooner or later sure to come at which the contractile waves cease to pass forward: they become blocked at that point. Moreover, the point at which such blocking of the waves takes place is extremely variable in different individuals of the same species. Sometimes the waves will become blocked when the strip is only an inch or less in length, while at other times they continue to pass freely from end to end of a strip that is only an inch broad and nearly a yard long; and between these two extremes there are all degrees of variation. Now if we suppose that the influence of the ganglion at the end of the strip is propagated as a mere muscle-wave along the strip, I cannot see why such a wave should ever become blocked at all, still less that the point at which it does become blocked should be so variable in different individuals of the same species. On the other hand, if we suppose the propagation of the ganglionic influence to be more or less dependent on the presence of a more or less integrated nerve-plexus, we encounter no difficulty; for on the general theory of evolution it is to be expected that if such fibres are present in such lowly animals they should not be constant as to position.

But there is a still stronger argument in favour of nerve-fibres, and it is this. At whatever point in a spiral strip which is being progressively elongated by section the blocking of the contractile wave takes place, such blocking is sure to take place completely and exclusively at that point. Now I cannot explain this invariable fact in any other way than by supposing that at that point the section has encountered a line of functionally differentiated tissue—has severed an incipient nerve.

Such, some of you may remember, was the state of the evidence when I last addressed you upon this subject. On the whole I provisionally adopted the view that all parts of the rudimentary muscular sheet of the Medusæ are pervaded by a plexus of rudimentary nerves, or "lines of discharge;" and I explained the fact of the tissues in some cases enduring such severe forms of section without suffering loss of their physiological continuity, by supposing that all the rudimentary nerve-fibres composing the plexus to be capable, in an extraordinarily high degree, of vicarious action. If you were to represent the hypothetical nervous plexus by a sheet of muslin, it is clear that however much you were to cut the disc of muslin with such radial or spiral sections as are represented in the diagrams, you could always trace the threads of the muslin with a needle round and round the disc without once interrupting the continuity of your tracing; for on coming to the end of a divided thread you could always double back on it and choose another thread which might be running in the required direction. And this is what I last year stated to be my opinion as to what took place in the fibres of the hypothetical nervous plexus;—whenever a stimulus wave arrives at a cut, I imagined it to double back and pass into the neighbouring lines of discharge, which I thus supposed to act vicariously for the divided line.

Such, then, when last I addressed you, was the standing of this question as to the character of these highly remarkable contractile waves. On the whole I decided in favour of a rudimentatary nervous plexus, notwithstanding the improbability that such a plexus should be capable of vicarious action in all its parts to so almost unlimited a degree.[1] I am glad to say that this decision has now been further justified by some additional observations which are of the first importance. For since my last lecture I have noticed the fact that reflex action takes place between the marginal ganglia of the Medusæ and all the contractile tissues of the animal. Thus, for instance, if you seize the polypite with a pair of forceps, the marginal ganglia almost immediately set the swimming-bell in violent motion—thereby showing that the stimulus must have coursed up the polypite to its point of insertion in the bell, and then down the sides of the bell to the ganglia, so causing them to discharge by reflex action. Again, suppose that seven of the eight ganglia have been removed from the margin of *Aurelia*, and that any part of the contractile disc is stimulated too gently to start a contractile wave from the point immediately stimulated, a contractile wave will nevertheless shortly afterwards start from the ganglion—thus showing that a *stimulus* wave must have passed through the contractile sheet to the ganglion, and so caused the ganglion to discharge. Indeed in many cases the passage of this stimulus wave admits of being actually *seen*. For it is a peculiarity of the numberless tentacles which fringe the margin of this Medusa, that they are more excitable than is the contractile tissue of the bell. Consequently a stimulus may be applied to the contractile tissue of the bell which is not strong enough to start a contractile wave in the bell-tissue itself, and is yet strong enough to start a contractile wave in the tentacles—one tentacle after another contracting in rapid succession until the wave of stimulation has passed all the way round the disc. The latter, of course, remains quite passive until the tentacular wave, or wave of stimulation, reaches one of the ganglia (or the single remaining ganglion, if the disc has been prepared by removing seven of the ganglia), when, after an interval of half a second for the period of latency, the ganglion is sure to discharge, and so to cause a general wave of contraction.

Now these facts prove, in a singularly beautiful manner—for this optical expression of the passage of a wave of stimulation is a sight as beautiful as it is unique—these facts, I say, conclusively prove that the whole contractile sheet of the bell presents not merely the protoplasmic qualities of excitability and contractility, but also the essentially nervous quality of conducting stimuli to a distance irrespective of the passage of a contractile wave. So I conclude there can be no longer any question that we have here to deal with a tissue already so far differentiated from primitive protoplasm, that the distinguishing function of nerve has become fully established.

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[1] This antecedent improbability is not so overwhelming as it is at first sight apt to appear; for we must remember that in a peripheral nervous plexus as we meet with it in the higher animals—*i.e.*, in the fully evolved form of such a structure—each of the constituent nerve-fibres is provided with an insulating coat for the very purpose of preventing vicarious action among these fibres and the consequent confusion among the reflex mechanisms which such vicarious action would manifestly occasion. But because insulation of peripheral nerve-fibres is thus an obvious necessity in the case of a fully evolved nervous plexus, it by no means follows that any high degree of insulation should be required in the case of an incipient nervous plexus. On the contrary, any hypothesis as to the manner in which nerve-fibres first begin to be differentiated from protoplasm must suppose that the conductile function of the incipient nervous tracts precedes any structure, such as that of nerve-coats, whereby this function is strictly confined to particular tracts. The antecedent probability being thus in favour of the view that insulating structures are a product of later evolution than are the essential nervous structures which they insulate, it would clearly be very hazardous to draw any analogy between an incipient nervous plexus such as I suppose to be present in the Medusæ, and a fully-evolved peripheral plexus of any of the higher animals. A less hazardous analogy would be furnished by the fibres which occur in the *central* nervous system of the higher animals; for here it may be said, both *à priori* from Mr Spencer's theory and *à posteriori* from histological indications, that the nerve-fibres occur in various degrees of differentiation. And that vicarious action is possible to some considerable extent through a bridge of the grey matter of the cord, has been shown by the double hemi-section experiments of Brown-Séquard. Moreover, the admirable experiments of Goltz would seem to indicate that vicarious action is also possible to a large extent among the ultimate elements of the brain. I may add that recent research has tended to suggest a novel interpretation of the way in which certain poisons, such as strychnia, act upon the cord; for whereas it has hitherto been supposed that the abnormal reflex excitability which these poisons engender is due to their exerting a stimulating influence on the cord, the researches in question have fairly well proved that the very reverse is true, viz., that the action of these poisons is to *depress* the vitality of the cord. For a number of facts go to prove that the abnormal reflex excitability is due to the impairment of some function which has been provisionally termed "resistance of the cord," a function which in health prevents the undue spread of a stimulus through the substance of the cord, and the impairment of which by the poison consequently admits of a stimulus spreading to an undue extent, so giving rise to the abnormal reflex excitability in question. As bearing on this subject, I may observe that while the action of strychnia on the Medusæ is the same as it is on the higher animals, viz. that of causing paroxysmal convulsions, it certainly seems to exercise a *depressing* influence on the tissues; for an extremely weak sea-water solution has the effect of blocking contractile waves in any part of a spiral strip that is submitted to its influence.—G. J. R.

THE NORWEGIAN ATLANTIC EXPLORING EXPEDITION

Tromsö, July 13, 1877

THE expedition met at the beginning with several unfavourable circumstances. In the last week of May Capt. Wille went out to Husö with the *Vöringen*, in order to determine the magnetical constants of the ship. After his arrival a flaw was discovered in the shaft, so that he went back to Bergen, where there was fortunately a new shaft lying ready. A few days later the ship was again at Husö, and was swung, not without some difficulty owing to rough weather. The *Absolute* magnetical observa-